

Uncertainty and the Federal Role in Science and Technology



*On April 4, 1994, Ralph E. Gomory spoke to Laboratory employees about the new role of the federal government in supporting science and technology. This article is based on Gomory's talk, which was presented as part of the Director's Distinguished Lecturer Series.**

THE federal role in science and technology has been much discussed in recent years. Considerable dissatisfaction is apparent on both sides: on the part of the federal government and on the part of the scientific community. The scientific community complains of inadequate or misdirected support. Individuals in government ask why scientific leadership has not been translated into economic or industrial leadership.

Some tend to characterize the scientific community as self-centered and self-serving.

Related discussions concern budgets, emphasizing certain applications, and setting scientific priorities. But priorities for what? What is it we are trying to do? What is the goal of all the effort and discussion?

Setting priorities can be most difficult if we do not have clear goals.

If we don't know where we are going, it is impossible to have a sensible discussion about the fastest way to get there.

I believe that a lack of agreed-on goals has complicated the discussion of scientific support. Thus, I will attempt to suggest some possible goals for various aspects of scientific support by the federal government. But first, it helps to understand some elements of the federal science scene.

*The Director's Distinguished Lecturer Series was inaugurated in October 1977, the outgrowth of a suggestion by the Continuing Education Committee's subcommittee on physics. Each year, about half a dozen well-known scientists are invited to LLNL as distinguished guest lecturers. The lecture series serves to acquaint Laboratory people with the eminent scientists and their ideas. It also provides an opportunity for those scientists to learn more about the Laboratory and its research.

Support of the Individual Investigator

By any reasonable standard, support of basic science—especially support of the individual investigator—has been the most successful of the federal government's roles in science and technology. A policy of support for basic science emerged in the post-World War II period. The great achievement of scientists during the war—for example, the atomic bomb and radar—gave politicians and the public a feeling for the immense power that resides in scientific knowledge. The thought that led to the policy of support, namely “Science is power,” was rewarded by scientific successes that have transformed and continue to transform the world.

One example is the transistor, an invention that grew out of the basic understanding of solid-state physics in the same way that the atomic bomb grew out of understanding the atomic nucleus. Another is molecular biology, with its remarkable revelations about the basic functions of all living things and the enormous and emerging consequences of this technology.

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When we seek to justify federal money spent on the individual investigator, we have, in reality, set an easy task for ourselves. We don't have to look ahead and speculate about individual research; we only need to look back at a great history of success. The idea of supporting the individual investigator works. The approach works, whether it is measured in terms of scientific progress or of advancing the material level of the world.

Despite that success, however, there are problems today within the basic science community itself.

Researchers face high rejection rates from the supporting agencies, such as the National Institutes of Health (NIH) and the National Science Foundation (NSF). We have seen a diminution of interest in science and engineering on the part of students. There is a long pipeline to the Ph.D. degree and difficulty in getting jobs at the other end of that long pipeline. Despite a remarkable record of success, we may not be producing a reasonable way of life for the scientist.

In trying to understand what is going on and what to do about it, we immediately encounter confusion and a great divergence of views. Some say the answer to the high rejection rate for grants is simple. Scientists clearly do good work; we should simply give them more money. We should fund any good idea because it's worth it. Others say that money spent on science has been increasing steadily, even accounting for inflation. To increase it more under the present ground rules will produce an ever-increasing population of research scientists who will be claimants for the same limited number of desirable jobs. More research scientists would mean still more competition for grants.

The remarkable fact is that we don't know what is going on. We don't have the most basic model of the process of generating researchers. As a result, what does happen is much more a political process than a thought-out process.

What we would actually do if we had a decent picture is also unclear. What would our goals be? Is it really possible to articulate goals for basic science even if we had a clear picture of what is going on?

Most of us automatically reject goals that set specific aims for scientific subjects. However, as a country, we could set goals in a different way. We could have a goal of being world-class in most major

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scientific fields. Today, we don't have such a process goal, and we don't even have a debate. I will return to this thought later.

What we should remember is that basic science and its support by the federal government has worked. It has benefited the world in obvious ways and should continue. However, we should also stop flying blindly toward an unknown destination for the good of researchers and the rest of the world as well.

Support for Megaprojects

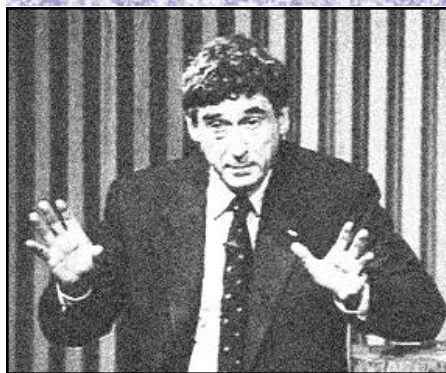
I distinguish between two types of megaprojects: those that I call real science, and those that are often referred to as science and justified as such but are not science.

Real scientific megaprojects include various orbiting telescopes, scientific satellites and space probes, and, until recently, the most prominent member of the group, the Superconducting Super Collider. These types of megaprojects often represent good science. But, do such endeavors represent the right way to prioritize and spend our science dollars? For example, we spend about \$2 billion a year on unmanned space probes. This is about the same amount of money that the NSF spends each year on individual investigators. Historically, the individual investigator has been far more productive.

Perhaps we could deal better with scientific megaprojects by incorporating their cost into the relevant scientific fields, such as astronomy, earth sciences, or physics. In this way, we could decide how we want to spend money to obtain world-class standing in a particular field. With such a goal, at least a sensible debate could ensue.

The second type of large project is what I call the nonscience megaproject. Space is the best example of this

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group. The space program originated in our race with the Soviets. Few who were around at the time will forget the extreme national reaction that greeted Sputnik. Edward Teller, in his usual picturesque way, asserted that we had suffered a defeat worse than Pearl Harbor. Out of this disturbed national atmosphere came a political decision to put men on the Moon. We did so to surpass the Soviets, not to settle the question of what the surface of the Moon is like.

Given this capsule view of the origins of the space program, we might wonder whether such a large program is necessary today. Our rivalry with the former Soviet Union has diminished. Its successor state, Russia, has abandoned communism and no longer represents a world-class ideological threat. Yet, we are still

spending more money on the space program (\$14 billion per year) than the combined budgets of three NSFs and one NIH.

If we ask whether the space program in its present form is necessary today, we would get more than one answer. We would be told, for example, that the program:

- Is important science.
- Recruits people into science.
- Contributes to civilian technology.

These explanations are all science- and technology-oriented, and they are all somewhat true. We might also be told—and here I think we are closer to the truth—that the manned exploration of space, and perhaps the eventual settling of space by people, is a national goal in itself, quite independent of science. But if exploring and settling space in this way is a national goal, then let us articulate that goal and debate it rather than obscuring it with scientific justification. If we accept this national goal, let us also decide to pursue it at a proper pace, which would not necessarily be the pace appropriate to a race with the former Soviets.

In contrast to basic science, space exploration, whatever its rationale, doesn't perform some obvious or useful function now in the absence of an intense American-Russian rivalry. For this reason, we need to clarify what we are doing. There is no scientific purpose that could justify the enormous bill. If the goal is actually something else, like manned exploration of space, let us articulate that as a national goal and then determine the pace and rate of expenditure that are appropriate for that goal.

Science in Support of National Goals

We have many national goals, although they are usually only dimly articulated. We have a goal of

making economic progress and of being economically competitive. We have the goal of improving the health of Americans and of protecting the environment. The goal that I know the most about is economic competitiveness.

In the U.S. in recent years, we have graduated from the idea that science alone guarantees industrial leadership to the idea that science and technology plus the rapid commercialization of new ideas are what matter. At the same time, the federal government has moved from a position of supporting only basic science to a position of supporting generic or precompetitive technologies.

Behind this shift is the thought that turning new technologies into real products is the issue. The notion is that we in the U.S. have the ideas, but others commercialize them. However, if the commercialization of new technology were really the problem, it would be very convenient because we could then use science and technology policy as a substitute for an industrial policy. Industrial policy, in a broad sense, is and has been a complicated and questionable subject in the U.S.

Unfortunately, this view of the problem flies in the face of the facts. The U.S. has not had an innovation problem to date, even in the sense of commercialization. The industries that make up the balance-of-payments deficit are textiles, automobiles, semiconductors, and consumer electronics. I know nothing about textiles, but the problems in the other three areas have had little to do with innovation. The problems have everything to do with manufacturing.

For these industries, it simply isn't true that we had the good ideas, and others commercialized them. In fact, they are all industries where U.S. companies commercialized the original ideas and grew to have a strong

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position in the mature field. However, they subsequently lost that position to competitive products with superior quality and lower manufacturing costs and to competition having a rapid development cycle leading to rapid, incremental improvement in the product.

To date, quality, speed, and manufacturing have been the real strength of the competition rather than the much-publicized advanced-technology efforts. Until we face that reality, we are unlikely to make progress.

In this area as in others, we need to set a goal—contributing to American industrial competitiveness through science and technology. We then need, in close cooperation with industry, to discover exactly what science and technology programs will actually contribute in the way of giving us competitive industry. We need to work backward from the competitiveness goal and the needs of industry rather than forward from the latest scientific event. Of course, there will be different views, but I believe a sensible outcome would emerge. The result is likely to be a mix of the old and new, of high technology and manufacturing technology.

In working toward this goal—contributing to industrial competitiveness—we must also consider the fact that there are several

very different situations in the realm of technology that call for different approaches. Prominent in the minds of academics and many in government is what I call the “linear model of technological progress.” In this model, an idea is born in science, it progresses through a technology stage into new products, and it gives rise to a new industry. The transistor went down that path a while ago. Molecular biology is evolving that way today. Here, we can imagine a government role in fostering the underlying science and possibly, but not certainly, helping new enterprises that may be struggling. The latter role is most plausible in areas that have a small market component. For example, the government might play a role in supporting work toward the cure of rare diseases where the projected income could not support the development effort.

A more difficult task is helping an already-established industry, such as semiconductors, where the issue is not new technology but the rapid, cyclic improvement of what is already there.¹ In this case, it is essential that industry participate from the beginning. Whatever is to be contributed must fit into an already-existing industry, its tools, knowledge, and plants.

Most difficult of all is the case where we would like to enter a technological industry that exists only outside the U.S. Here, fostering technology is not enough. Even if we understand liquid crystal displays, for instance, being able to manufacture and market them competitively is a quite different matter. Technology is only part of a much larger game, and here we are on the fringes of true industrial policy.

Today, in working toward a competitiveness goal, we are largely in the realm of experiment. We have some new programs, like those of the Department of Commerce and Sematech (a consortium of U.S.

semiconductor firms established to compete more effectively in the global marketplace). Then there is the large and daunting problem of turning some of our national laboratories to a new direction in support of competitiveness or some other national goal. Once we have clarified our goal in this area, and once we decide that we need to work backward from that goal and see what is needed, experimenting will certainly be the right thing to do.

Setting Goals for Science

In looking at the present federal effort, we have seen how the support of basic research became possible after World War II, how the space program emerged from our rivalry with the Soviet Union, and how the exigencies of competitiveness are having some effect on the federal science and technology scene. The situation today has emerged from a normal historical process. However, we should ask ourselves whether the historical motivations are still correct, and even if they are, how correct. Even if most of us agree that government support of basic research is an idea that made sense in the past and makes even more sense today, that alone does not answer the question of how much basic research is enough.

What I have to say on this question is based on some ideas I have been pursuing for some time. It is also based on the recent (1993) report issued by the National Academy of Sciences, the National Academy of Engineering, and the Institute of Medicine entitled "Science, Technology, and the Federal Government: National Goals for a New Era."²

Basic research is funded because of the belief that something broadly and directly useful will eventually come of the scientific effort. Scientists do not often think about usefulness, but scientific funding ultimately rests

"Scientific funding ultimately rests on society's hope and expectation of practical results."



on society's hope for and expectation of practical results. This expectation has been historically fulfilled in that basic science has already provided major practical returns, such as the transistor and other examples given earlier.

However, the overall success of basic research has not prevented people from wanting to fund only those areas within science that can be seen to be useful. At the same time, such individuals question the support of areas that do not seem to be useful. In contrast, scientists have generally wanted funds to do what they think matters. They often decry research funding directed at useful goals as misguided and shortsighted.

I think that both of these views represent partial truths, and neither is the whole story. To see why, I would like to introduce the following "uncertainty principle" for scientific funding: We can see when some area of science is useful or is about to be useful, but we can't see that some area of science will be useless.

Consider the first half of this sentence. Some fields of science

demonstrate their practical potential in a clear way at a certain point. Molecular biology today, and for some time in the past, is an example. In fields like this, the U.S. may well decide that it wishes to lead the world and be the first to benefit from the useful consequences. The practical consequences—the usefulness to society—can take many forms. They might be contributions to economic competitiveness, to national health goals, or to national environmental goals. Historically, the practical consequences have often been contributions to national defense goals.

Note that the benefits from world leadership are outside science itself. They have to do with the goals of society, not with whether one field of science or research is more exciting than another.

Now consider the second half of the sentence: We can't see that some area of science will be useless. This statement is more than something scientists merely want to believe because it justifies their pursuit of whatever they want to pursue. It is also a reality. The history of quantum mechanics is a good example.

In the 1920s, there was no subject more pure and more esoteric than quantum mechanics. At first, we had the uncertainty principle and the baffling puzzle of electrons that behaved like waves one moment and particles the next. Quantum mechanics was a subject with exciting scientific and even philosophical impact, but nothing could have been farther from real applications. By the 1930s, quantum mechanics began to have an effect on the field of solid-state physics. After the war, we gained an improved understanding of the fundamentals of crystalline solids, which led to a better grasp of the role of trace impurities and their effect on the flow of electrons. The transistor was not far behind. The transistor

had a tremendous impact on computers and on electronic devices of every sort. These devices now affect the everyday life of us all. Not much more than 30 years separated the esoteric and apparently useless from the enormous impacts we now experience each day.

This example shows that practical discoveries do, indeed, turn up in the course of pursuing the most basic knowledge. What is more, we can expect the process to keep happening. Sometimes, people outside science think that the process is pure serendipity, that if we turn over enough rocks, every now and then we will find a diamond. However, the actual process is nothing like that. In reality, it is the systematic exploration of a significant piece of the natural universe. It is not surprising that when we begin to understand, in a fundamental way, important pieces of the universe—for example, how solids hang together or how living beings function at the molecular level—at some point, the understanding will allow us to do things we couldn't do before.

The two halves of the uncertainty principle lead to these two consequences³:

1. The U.S. should maintain clear world leadership in some selected areas of science.
2. The U.S. should be among the world leaders in all major areas of science.

The first conclusion is the clear recognition of the demonstrated usefulness of a scientific field. The selection of fields for world leadership is a social, not a scientific, judgment. It is a judgment that money spent on a selected area will give a large social return.

The second conclusion is the explicit recognition of two things:

“We can see when some area of science is useful or is about to be useful, but we can't see that some area of science will be useless.”



the unpredictability of basic research, and the fact that scientific knowledge is not a free good. We cannot benefit from scientific research, even in a world in which scientific communication is both free and international, unless we have paid the price of being a significant participant.

If the U.S. is among the world leaders, when something happens anywhere in the world and a field begins to show practical promise, we would be in a position to participate. For example, the possibility of high-temperature superconducting materials suddenly appeared a few years ago in the work done in Zurich, Switzerland. These materials had the promise of cheaper electricity and many other applications. Americans were major participants in the field almost at once. The U.S. should always be at least in that position.

Now let us apply this way of thinking to two current examples. First, consider the Superconducting Super Collider (SSC). Is particle physics a field where, because of its clear contribution to society, we must be out ahead of the rest of the world? If we want to be clear leaders, we should build our own SSC. If we are content to be only among the leaders, we should try to work out with the Europeans a cooperative arrangement to advance the field. The question is this: Do we need clear leadership in particle physics for societal reasons? The answer is not a judgment to be made by scientists, although it needs scientific input. The simplest test is to ask if there is a large and demonstrated payoff from the field in terms of its contribution to the economy, medicine, or any other such societal goal.

I think that the record of particle physics, to the extent that I know it, simply does not support the notion of a large societal payoff from building the SSC. Nor is there any reason, at the moment, to suppose that the future will be sharply different from the past. I would personally conclude that this is not a field for clear U.S. leadership, that the SSC should never have been started, and that we ought to go back to the drawing board and see if we can work out something with the Europeans that will allow us both to move forward in particle physics.

Turning again to the topic of molecular biology, I think we would come to the opposite conclusion. This field has a clear relation to an emerging industry as well as applications to health. This country might well decide that, in the interests of national health and of the emerging biotechnology industry, we want to be well ahead of the rest of the world.

Other Consequences and Conclusions

The goal of being among the leaders in a given field is a measurable goal. It involves a comparison of the level of science in the U.S. in a particular field with the level of science for that field in other countries. We are among the world leaders if we are roughly on a par with the work done abroad. Of course, many other questions need to be answered as well. For example, do we compare ourselves with other individual countries or with Europe as an entity? Such questions need to be worked out in accord with the basic issue of whether we are in a position to react and participate if the field suddenly changes.²

Note that the stress here is on a comparison not merely with other countries, but a comparison within a particular field. Testing whether we are among the leaders in a given field of physics—such as condensed-matter physics—does not call for a comparison of funding for condensed-matter research with funding levels for a

different field of physics or some field within chemistry. It also does not call for arguments about whether one field is more exciting than another. It says we should measure ourselves against the world standard in each of these fields.

In addition, we do not need to make a comparison of big science with little science. The goal of being a leader—or a clear leader—should establish the mix of big science and individual investigator science in that field. The mix that is right for leadership in particle physics surely is not right for leadership in condensed-matter physics. What matters is to get it right for each field, not to add up the big science and the little science across the board and make a meaningless comparison of the totals.

I think the time is right for a new era in federal support of science and technology. I think it is possible to clarify where we are going to set the goals and how we are going to work toward them, while at the same time respecting the many unknown outcomes from basic research. If we do this, society will benefit even more than it has in the past, and science itself will be supported in a more stable way.

Key Words: *science and technology*—basic research, federal support, goals, individual investigators, large projects.

Notes and References

1. R. E. Gomory and R. W. Schmitt, "Science and Product," *Science* **240** (4856), 1131–1132, 1203–1204 (1988).
2. Committee on Science, Engineering, and Public Policy, *Science, Technology, and the Federal Government: National Goals for a New Era* (National Academy Press, Washington, D.C., 1993); available from the National Academy of Sciences, 2101 Constitution Ave., N.W., Washington, D.C., 20418.
3. These are the two main conclusions of Reference 2. These concepts are also closely related to the following: R. E. Gomory, "Goals and Priorities for the U.S. Government's Role in Science and Technology," Testimony before the House of Representatives Subcommittee on Science (Committee on Science, Space, and Technology, April 28, 1992); R. E. Gomory and H. Cohen, "Science: How Much is Enough?" *Sci. Am.* **269** (1), 120 (July 1993); R. E. Gomory, "Goals for the Federal Role in Science and Technology," *Phys. Today* **46** (5), 42–45 (1993).

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